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Interactive comment on "Responses of an abyssal meiobenthic community to short-term burial with crushed nodule particles in the South-East Pacific" *by* Lisa Mevenkamp et al.

Anonymous Referee #2

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The manuscript describes results from an experiment to assess the combined effect of burial and manganese nodule particles on abyssal meiofaunal communities. I though the manuscript was very interesting, and written by a rising star in deep-sea ecology. The paper and data will be very useful to academics as well as policy organisations dealing with the effects of sediment and nodule particle deposition from deep-sea mining for polymetallic nodules. My main concern about the manuscript is that the substrate addition didn't appear to have a huge impact on benthic community structure in the experiments. While these are the results that have been collected and need to be reported, my feeling is that a lot of the fauna in the substrate addition treatment were actually dead but hadn't decomposed at the end of the experiment. Then, when

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the fauna were preserved in formalin after 11 days everything that was alive and dead at the end of the experiment was preserved such that no change in community composition could be detected. I understand that this is difficult to assess using staining methods (as stated in the discussion by the author), but it would have been possible to assess the condition of some of the meiofauna at the end of the experiment (e.g., by looking at the appearance of the striated-muscles of the harpacticoids from the burial treatment, and comparing with the control samples). Similar approaches have been undertaken in the past (see Thistle et al. 2005 , Mar. Ecol. Prog. Ser. 289: 1-4) to estimate the proportion of meiofaunal harpacticoids killed in situ by CO2 perturbations. I would suggest that the lack of information about meiofaunal death is clearly flagged as a possible reason why differences in benthic community composition could not be detected. Although the authors went some way to discuss meiofaunal death in their discussion, this point really needs to be stressed. This is because, at present, mining contractors may use this paper to state that manganese nodule particle/ sediment deposition does not alter benthic community composition, and I am not convinced this will be the case.

I recommend that the article be published eventually following some moderate revisions.

Minor points to consider: Abstract 1) Line 11: change to “may rive the extraction of deep-sea mineral. . .” 2) Line 13: Change to “Experimental studies are scarce and simulated effect studies are small scale relative to the effects that will be seen during deep-sea mining. . .” 3) Line 16: Insert “in 2015” after conducted. 4) Line 22: Remove “original” Introduction: 1) Page 2, Line 10: It would be good to provide the range of typical manganese nodule growth rates here, because <250mm myr-1 can mean 0.00000000001mm myr-1 to 250mm myr-1. 2) Page 2, Line 15: What about organic matter dilution as well 3) Page 2, Line 18: change “of” to “from” 4) Page 2, Line 22: It would be good to give the reader some idea about the natural sedimentation rates in the abyss, and some indication of the levels of sedimentation that will occur during deep-

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sea mining. 5) Page 3, line 2: Change to “which causes at worse, meiofaunal death, but at least removal. . .” 6) Page 3, line 24-26: I am confused as to why the amount of metal in the animal tissues is a robust way to assess toxic effects. You could have an animal with a high level of metals in its tissues, but the animal is highly resilient to metal toxicity. Therefore, the amount of metal in its tissue does not really always show the degree of toxicity from that particular metal. Methods: Overall methods. Did you assess the volume of the sediment taken up by solid nodule particles in your 10cm2 sample from the controls and burial treatments. If some sediments have more solid nodule particles, then there is less sediment to inhabit and this may have an effect on the densities that you found. 1) Page 4, line 8: You need to mention how you sampled the nodule and crushed the nodule to make the substrate. This information is missing. 2) Page 5, line 8: Change to “The second push core was used to. . .” 3) Page 5, line 9: Did you try and get an idea of the organic matter quality of the sediment and the added substrate? Given that a lot of meiofauna directly consume labile microbial organic matter (see Bernhard & Bowser. 1992. Mar. Ecol. Prog. Ser. 83: 263-272, Ingels et al. 2010. Mar. Ecol. Prog. Ser. 406: 121–133.), the quality of the substrate, as well as the effects from burial and the content of the manganese substrate may have all had an influence on the meiofaunal response. If you do not have actual Chl-a, or lipid concentrations, you can at least get an idea from the C: N ratio. 4) Page 6, line 18: Please define “live time”. It sounds cool, but I have no clue what this is. 5) Page 7, line 1: What Simpson metric are you referring to? The term ’Simpson’s’ can actually refer to any one of 3 closely related indices (Simpson’s Index, Simpson’s Index of Diversity or Simpson’s Reciprocal Index). 6) Page 7, line 1: What univariate analyses were used? Results: 1) Page 10, line 13: I think that the biodiversity metrics being the same in both the burial and control treatments may be due to you not being able to differentiate between live and dead fauna. This could have been assessed in the harpacticoids by looking at the condition of the fauna, since dead fauna would appear more degraded even if they’ve been at abyssal temperatures for a few days. As I stated before, it is important that the manuscript is carefully worded to reflect this as this result could be used as evidence for

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no impact from re-sedimentation of sediment and nodule particles during mining, and I doubt this will be the case given the low background sedimentation rates in the abyss. 2) Regarding my first point in the methods section above, it would probably have been a good idea to standardise your meiofauna abundances to per unit volume of sediment rather than area. If the nodule substrate layer was full of cm-sized particles then the amount of living space available to the nematodes would be significantly less than in the control samples. Standardising the abundances to unit volume (if you have the data) may show much larger differences, and you may detect differences in community structure, or abundance (at least) between treatments. Discussion: 1) Page 13, line 19: Given the coarse nature of the nodule particles, wouldn't O2 penetrate through the manganese substrate layer relatively easily. I understand there is burial, but diffusion will be dependent on the porosity, which should be greater in the substrate layer. 2) More overall impression of the discussion is that the authors need to acknowledge the weaknesses of the study (e.g., being unable to document meiofauna death) to a much better degree. This is done somewhat, but it really needs to be emphasized that a lot of the responses seen (or lack of them, e.g., in the biodiversity data) may be caused by the inability to distinguish living from dead fauna in the different treatments.

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